Biogeosciences Discuss., referee comment RC1
https://doi.org/10.5194/bg-2021-118-RC1, 2021

Comment on bg-2021-118

Anonymous Referee #1

Referee comment on "Climate pathways behind phytoplankton-induced atmospheric warming" by Rémy Asselot et al., Biogeosciences Discuss., https://doi.org/10.5194/bg-2021-118-RC1, 2021

Review of “Climate pathways behind phytoplankton-induced atmospheric warming” by Asselot et al.

In this study, the authors explore the sensitivity of globally-annually-averaged atmospheric CO2 concentrations to phytoplankton induced surface ocean heating. Phytoplankton heat the surface ocean by absorbing radiation, which then heats the surrounding seawater and leads to changes in heat and carbon transfer with the atmosphere. Predicting even the sign of the change can be difficult since multiple physical factors (including changes to circulation) and chemical factors (changes to solubility) are relevant. The authors disentangle the relative strength of phytoplankton-mediated heat versus carbon transfer for influencing atmospheric CO2 via a series of idealized experiments in which physical and chemical factors are controlled. They find that phytoplankton heat absorption has a stronger influence on carbon exchange than heat exchange.

The study promises to be an excellent contribution to the question of how important the inclusion of phytoplankton heat absorption in climate models might be. Adding new code to existing models requires effort, and this study suggests this functionality alters heat and carbon fluxes and atmospheric CO2 concentrations. In their idealized framework, the effect on atmospheric CO2 is only 9 ppm on global annual average, suggesting phytoplankton heat absorption is a minor contributor. However, as the authors point out, this estimate may be a lower bound in less idealized conditions or the real world.

The experimental setup is thoughtfully constructed. Overall, results are discussed appropriately, although I found some critical information to be missing both from the description of the experimental setup as well as in the analysis of the model results. The paper is presented clearly, though I would like to see a modest expansion of the conclusions section to bring together the various experiments (including the sensitivity tests) in a more meaningful way.

I have some specific comments that should be addressed to improve the clarity of the manuscript:

- Figure 1 shows that the carbonate counter-pump is simulated, but the details of this and its implications and effects on the results are never discussed. Please provide the reader with more detail on this aspect of the biophysical feedback on carbon exchange.
- Depending on model formulation, phytoplankton-mediated heating of the surface ocean could have an effect on the top-down control of zooplankton on primary production. Please discuss if and how this might affect results.
- The absence of nitrogen cycling is not discussed but could have additional consequences not modelled here. If phytoplankton warm a low-oxygen region, and this causes additional oxygen consumption, then there might be additional denitrification. More denitrification would lead to more nitrogen fixation downstream, which might increase biomass regionally (a change in the spatial pattern of NPP, which affects the overlap of solubility vs biomass) and therefore any pathway sensitivity of atmospheric CO_2.
- How is wind stress forcing treated in the model? If it is like Weaver et al. (2001) then there is a change in the wind stress with a change in global temperature. I could not find this information in the manuscript and it has important implications for the results.
- What are the temperature dependencies in BIOGEM/ECOGEM? Is there an approximation of a microbial loop? Is remineralization accelerated by warming? I could not find this information. This is related also to point (2).
- L175-181: Doesn't the application of ECOGEM change the biogeochemical distributions in the model? If so, 1,000 additional years of spin-up might not be enough. Are the sensitivity tests applied after the 10,000 years + 1,000 years, or are they applied after 10,000 years (is the atmospheric CO_2 prescribed for the 1,000 year ECOGEM spin-up)? Is atmospheric CO_2 allowed to stabilize in all model simulations, or are all simulations only run 1,000 years? More details on experimental setup would be useful here.
- It is not clear whether Fe is a prognostic variable. If it is, then are there temperature effects on Fe solubility (and therefore, bioavailability)?
- Section 2.3: Does sea ice have no influence on heat exchange? Please explain.
- Table 2 and Section 4.1. I don't see these as being very important to the main message of the paper and suggest moving them into the Appendix. However, it is interesting comparing Tables 2 & B1. Inclusion of seasonal cycling has more of an effect on SST than 40 ppm change in CO_2! What is the effect of seasonal cycling on chlorophyll?
- Section 4.2 could also move to an Appendix. But, as mentioned elsewhere- is the wind forcing different across the main model experiments due to differences in SST anomaly from pre-industrial state?
- Conclusions are missing some wider speculation as well as more discussion of model limitations. What would the authors expect if a land model were to be included in their simulations? What about sea ice influencing heat flux, or carbonate counter-pump effects or changes to micronutrient availability? Feedbacks in a transient state?

Finally, I have some minor suggestions for language:

L1: "in which ways", or "ways in which"?

L10: "…the freely evolving solubility of CO2…" (due to what? Is it going up or down?)

L11: Some kind of summary sentence that gives the results context would be useful here.

L20: "evidence supports"

L29: "Models of differing complexity…"

L61: "…as follows…"

L71: "…composed of…"

L75: "…the sensitivity of atmospheric CO2 is mainly explained…"

L119: remove "availability"

L119: "prey"

L119: Table A1 shows that phytoplankton are ~3X smaller than the zooplankton. Does this mean there is no zooplankton grazing in the model?

L125: "For simplicity…"

Eqn 1: Does sea ice not affect light attenuation? Why not? Does this mean there is biomass under sea ice?

L132: "…total chlorophyll concentration .."

L142: "…is released in the form of…"

L151: “…received…”

Figure 1: There is no arrow between sea ice and anything else

Figure 5: Shouldn’t “Bio” look more like Figure 1 (with an arrow going from SAT to SST?) Plus, CO_2 should be able to enter the ocean in this simulation? Same for BioLA.

L203: Is this only the biological pump? What about $CaCO_3$?

L216: “…concentrations differ.”

L225: “…we ensure that the heat and CO_2 interaction is negligible by …”

L227: “…analyses…”

L237: split into 2 sentences

L245: “Finally, the response of the surface atmospheric temperature due to changes in oceanic and atmospheric properties is studied”

L250: What is the temperature dependency that produces the shallower flux of OM due to warming from phytoplankton light absorption? (For those not familiar with your model).

L251 (and elsewhere): “The chlorophyll biomass difference…” (Also applies to SST)

L264: remove “more important”

L 270: might this be due to the lack of an influence of sea ice on heat flux?

L 275/76: Is there a difference between HCorgSol and HCorg w.r.t. $CaCO_3$ production?

L 296/97/300: I think “more important” is not what is meant. “Larger” or “Greater”?

L 307: “…which increases…”

L 308-310: split into 2 sentences

L319: ”…heat flux, explaining…”

L330: replace “pointing out” with “which indicates”

L332: “to outer space”

L362: ”…, where the lower the humidity the higher the evaporation rate.”

L371: remove “indubitably”

L375: maybe not “clearly”, since I still have some questions about experimental setup and model assumptions beyond seasonality.

L388: ”…identical responses…”

L 390: “…smaller increase…”